

## The Coloration Problems.

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These remarks are intended in some measure as a reply to the paper published by Mr. Colthrup in the May, 1912, number of the *Ent. Record* (page 124, *et seq.*), and the further paper by Lieut.-Col. N. Manders in the July-August, 1912, number (page 171, *et seq.*). The tardiness of my rejoinder should be explained. I had penned an article, and it actually got as far as a printer's proof, when the impending publication in the *Trans. Ent. Soc. Lond.*, (1912), page 445, *et seq.*, of Lieut.-Col. Manders' results of temperature experiments on *Hypolimnas misippus* and *Danaida (Limnas) chrysippus* rendered it desirable in the view of the Editorial Committee that I should consider those results before publishing my remarks. As is the way in these matters it was difficult to follow Lieut.-Col. Manders' argument without reference to the *Proc. Zool. Soc. London*, (1911), page 696, *et seq.*, where he has expressed his views at length, and this likewise led me to read Mr. R. I. Pocock's *Results of Experiments* on page 809, *et seq.*, of the same publication, and to reread Prof. Poulton's *Essays on Evolution*. The perusal of these papers led me to consider it desirable to extend and modify my original remarks, both out of respect for the industry and ability which Lieut.-Col. Manders had expended on experiment, and as a recognition of the fact that the evidence he has brought forward is in some respects contrary to my own experience, and in others not helpful to the establishment of the coloration theories on that firm basis of evidence that those of us who believe in them would like to have.

I hold strongly that the contested theories are the only correct and natural explanations of the observed phenomena; but the attacks upon them have revealed weak spots in the arguments deduced in their favour, and have shown that in some respects the evidence is slender or negative; therefore when possible one should justify one's faith by examples fresh from the field, rather than by logic evolved in the laboratory and the museum.

It is a little difficult to make an effort to reply to various different papers and yet retain some logical sequence to one's own remarks. However, at the risk of making this seem very disconnected, I shall deal with the matters I have to remark upon in the order in which they come in the papers referred to, simply remarking that I do not pretend, or assume, that I have dealt with the papers fully or adequately.

With regard to Mr. Colthrup's photographic test, I would remark that that is a very bad test as a rule, unless an infinitude of care be expended. The record on a photographic plate is one of luminosity value and not of colour or even monochromatic values as affecting the human eye. An autochrome photograph would be a far better test than an ordinary plate, but there again the human eye (and probably the avian eye) perceives shadow as being a different colour to that recorded by, and relieved with a greater abundance of detail than shown in the photograph. This is so even when one uses the best panchromatic plates manufactured and the most perfectly adjusted orthochromatic screens. I know by experience, that to secure any detail in  
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one's high lights, one has to chance a dense black shadow in monochrome, or a vivid blue shadow in autochrome, where the eye sees the detail and colour subdued by shadow, it is true, but neither the monochromatic black nor the autochromatic blue. (It is not true that near shadows are *per se* blue or purple to the eye, as some artists contend; distant shadows appear to be so, owing to water vapour in the atmosphere; but in dealing with these problems it is near objects that are to be considered.) The result is that many objects stare at one from a photograph that are unobtrusive in nature.

The term "protective resemblance" to which Mr. Colthrop objects, is a rough and ready way of expressing the presumed utility of the coloration to its possessor, and if the coloration theories (or in fact the Natural Selection Theory itself) be correct, the name is not a bad one, at the same time it is incomplete and in some ways misleading.

The vocabulary suggested by Prof. Poulton in *Essays on Evolution*, page 226, is in every way preferable. In fact Mr. Colthrop's instances of the elusive collar-stud and the forceps as examples of "resemblance" are really examples of the confusion of thought the term introduces. Neither bears the slightest resemblance to its surroundings, and the difficulty arises from the bizarre coloration of the surroundings themselves. The optical centres transmit to the brain so many conflicting stimuli, arising from the many different forms and colours observable, that the brain cannot sort them out rapidly enough and clearly enough to deduce either the form or the colour of the missing object. Cryptic coloring as it becomes more perfected takes on in some degree the normal coloring, chequered or otherwise, of the surroundings, making it still more difficult to deduce, from the stimuli received by the optic centres, the nature of the object seen. Variegation of colour alone tends to inconspicuousness, quite apart from whether the colours and surroundings match or not.

A dull grey is very difficult to see unless it be in a big mass. When the mass is sufficiently large, if the grey be broken up, however crudely with colours however bright, the mass is the more difficult to see. So pronounced is this that the big guns in some of our South Coast forts, which are all colours of the rainbow, are very difficult to see at a very short distance. Even the black and white parti-coloured buoys round our coast, so painted in the hope they would be thereby rendered conspicuous, are lost to sight much more quickly than a plain black or plain red buoy.

The eye certainly takes in the colour, but the brain fails to deduce the form and nature of the object so concealed, for it loses its chief guide to form, *viz.*, shadows. (Confer Prof. Poulton's remarks on the inclined attitude of rest adopted by the Satyrids with cryptic undersides, which remarks are entirely in accord with my own experience, and Dr. Longstaff to the same effect, *Trans. Ent. Soc. Lond.*, 1908, page 647, *et seq.*). If Cryptic Coloration does not serve its possessors as a means of escaping attack at rest, it is beyond my imagination entirely, either as to how that coloration ever arrived at its present perfection, or as to the manner in which it could be useful to its possessor. (Assuming that a utility to its possessor be necessary at all, to which proposition I shall advert later). (I note Lieut. Col. Manders appears to agree with my views on this head).

Mr. Colthrop says, that he cannot see that it has been proved that

lepidopterous imagines are subject to the attacks of birds or other enemies when at rest in the day time, yet he admits on page 122, only two pages before his remark, that certain *Biston hirtaria* were dragged into crevices by spiders, and it is my own experience that I often find behind loose bark, when pupa digging and the like, the dismembered wings of the victims of attack by spiders.

On July 8th, 1910, in the New Forest I witnessed a centipede, rush out of a crevice in the bark of a tree, seize a *Tortrix ribeana*, that was imbibing sugar, and rush back with its victim in its jaws. (The captor and prey were sent to Professor Poulton).

On July 4th, 1908, in Berewood, I witnessed a similar attack by a large beetle (species ?) on *Xylophasia polyodon*, resulting in the death of the latter insect. Both cases I admit were evening cases, and neither lepidopterous insect could strictly be said to be at rest, as both were early visitors to the sugar patch; both, however, were motionless, and both must have been seen at quite a little distance. The *Tortrix* at 9 or 10 inches, the *X. polyodon* at over a foot.

In both papers in the *Ent. Record* the writers seem to desire to limit the enemy predicated by the theories to birds, and to test the theories by attacks on one order of insects only. Whilst I think this is an entirely untenable position, yet as birds are to be the factor and Lepidoptera the order, let us examine the matter from that point of view.

Mr. Colthrup says he finds that it is the exception for birds to attack. Lieut.-Col. Manders holds the same opinion (although it appears to me somewhat faintly), but notwithstanding the comparative paucity of recorded evidence, I cannot quite agree with either gentleman. Neither one seems to me to be fair to the amount or character of the evidence that has been compiled, notwithstanding that the latter of them, at any rate, has quite a share of recorded evidence to his credit. Neither seems to give due weight to the fact that this volume of evidence, once its necessity appeared, has been daily increasing in magnitude and that with rapidity. Do these gentlemen, when they say that the birds, with the possible exception of the Tits and the Kestrel, (which latter I gratefully acknowledge), do not systematically search for Lepidoptera, understand the psychology of the birds themselves, and have they given full weight to their particular habits? We actually know very little about what passes in the minds of the birds, and we do not know much more about the capabilities of their optical apparatus. It by no means follows that a moth, conspicuous to the trained eye of the entomologist, backed by his superior deductive capacity, is conspicuous to *Parus palustris* var. *dresseri* (the British Marsh Tit) when he is looking for his breakfast.

Mr. E. Harker Curtis, my brother, is an ornithologist first and a lepidopterist second, so I am possessed of a tolerable knowledge of our British Birds and he of our British Lepidoptera, and since this question was raised some years back, we have, so far as our restricted leisure permitted, noted the birds' feeding habits. One thing there is, which we have long suspected and that we are now quite convinced of, and that is that the birds will not, as a general rule, feed properly if they know that they are being watched. They will pretend to feed, but all the while the pretence covers the fact that they are watching you. Occasionally one gets one's bird unawares, and then as often as

not one's bird is catching an insect. (Mr. Pocock's experience *Zool. Soc. Lond.*, 1911, page 810, bears this conviction out, since birds are quite as shy outdoors as indoors, and I note Dr. Chapman holds the same opinion as we have arrived at *Proc. Ent. Soc. Lond.*, 1909, viii.)

Mr. Colthrop surmises that birds rely on their beaks when looking for insects, again I cannot agree. I have watched Tits and other birds feeding for many hours with the aid of a powerful pair of x30 binocular telescopes by a maker of admitted reputation. I find that all searching birds rely on eyes and ears almost exclusively. (I here remark that I do not include the long billed *Charadriiformes*). Dr. A. G. Butler remarks, *Trans. Ent. Soc. Lond.*, 1910, p. 153, "he (sc. the bird) sees the legs and immediately approaches and pecks it." That is it uses its eyes first. The Thrush (*Turdus musicus*) certainly relies on eye and ear alone; when worm hunting I believe on ear, when insect hunting mostly on eye.

The Tits again rely entirely on the eye, using the bill only for testing what their eye has revealed, and they certainly conduct a detailed minute and exacting search of all the bushes, small boughs of trees, horizontal limbs of trees, horizontal rails of palings, etc., in their vicinity (not trunks as Mr. Colthrop suggests), such a scrutiny that one would almost think that the most perfect cryptic coloration would fail to defy it.

The Nuthatch (*Sitta caesia*), the Tree Creeper (*Certhia familiaris*), the Woodpeckers (*Geococcyx viridis*, *Dendrocopos major* and *D. minor*), on the other hand, affect the trunks and large limbs almost exclusively, and they too scrutinize and do not use their bills till something attracts their attention. With the Woodpeckers, however, they do deliberately knock off chunks of bark and then scrutinize the area laid bare. My brother and I watched *Dendrocopos major* (the Great Spotted Woodpecker) doing this in the Spring (at very close quarters indeed, fortunately unbeknown to *D. major*, who failed to detect our proximity for quite ten minutes).

The Warblers and Gold-crests seem, too, to rely entirely on sight, but also seem to me to confine their search almost exclusively to the leafy parts of trees and bushes.

*Daulias luscinia* (the Nightingale), a dire enemy of insect life, I must reluctantly put on one side. He searches the undergrowth, but is so shy and so retiring that I honestly cannot say that I can call to mind a single instance in which I have seen this bird in active pursuit of food.

*Erithaca rubecula* (the Robin) relies on eye, and is a ground and tree trunk feeder. The Chats *Saxicola oenanthe* (the Wheatear), *Pratincola rubicola* (the Stone-chat) and *P. rubetra* (the Whinchat) are ground and small bush-feeding birds, and undoubtedly rely on eye alone. *Passer domesticus* (the Common Sparrow) searches by eye alone, of that I am convinced.

With regard to birds that kill on the wing, they one and all rely upon eye I believe. I am ready to admit that they may hear the rustle and click of an insect's wing, as I used to be able to hear it, but can, alas! no longer do.

Rev. K. St. Aubyn Rogers points out, *Trans. Ent. Soc. Lond.*, 1908, page 498), that tropical butterflies rest at a time when tropical birds are most active in pursuit of food, and it should be borne in mind